

# Remarks on Consistent Histories and Bohmian Mechanics<sup>†</sup>

ADRIAN KENT

*Department of Applied Mathematics and Theoretical Physics,  
University of Cambridge,  
Silver Street, Cambridge CB3 9EW, U.K.*

## Abstract

Recent work with Dowker on the scientific status of the consistent histories approach to quantum theory is reviewed and summarised. The approach is compared with formulations of quantum theory, such as Bohmian mechanics and the Copenhagen interpretation à la Landau-Lifshitz, in which classical variables are explicitly appended. I try to explain why the consistent histories formulation is scientifically problematic, in that it is a very weak theory, but also scientifically interesting, shedding new light on quantum theory.

<sup>†</sup> Published in *Bohmian Mechanics and Quantum Theory: An Appraisal*, J. Cushing, A. Fine and S. Goldstein (eds), Kluwer Academic Press (Dordrecht, 1996). The article refers to discussions at the 1995 Bielefeld meeting, “Quantum Theory Without Observers”.

A distressing feature of discussions of the problems of quantum theory is their tendency to transform physicists from thoughtful and sophisticated scientific critics into uncomplicated partisans or unsympathetic spectators. This holds true although the most interesting questions posed by the various formulations of quantum theory, and by rival theories, are precisely the type of scientific and technical problems which physicists are trained to address. What, for example, does any given theory or formulation allow us to predict or infer, and from what data? Which of these predictions and inferences can be tested? How precise is the mathematical formulation, and what mathematical properties does it have? To what extent is it consistent with important physical principles such as invariance under Lorentz or general coordinate transformations? How elegantly is the theory formulated? On how many arbitrary quantities does it depend?

It would be good to reach consensus. Our present lack of success seems to stem less from subtle difficulties or metaphysical differences than from the fact that scientific assessments of interpretations of quantum theory or its rivals are unfashionable. I do not want to overstate the importance of scientific appraisal. Of course, creative theoretical work is the life blood of physics; physicists need not, and perhaps generally should not, also be philosophers of science. But it is, surely, good to have a clear understanding of what current approaches to quantum theory can, or could possibly, achieve. And, in fact, I would suggest that it is now relatively easy to see that every approach to quantum theory leads to serious problems and that there remain relatively few research programs with any serious ambition of solving our difficulties. This, certainly, was the majority view at Bielefeld, though agreement on precisely which are the serious programs and problems was harder to come by. A minority view, forcefully put at the meeting, was that the problems of quantum theory have been solved by the consistent histories formalism — or at least that the form of a solution has been convincingly sketched. My impression at the end of the meeting was that, although most of the participants believed this to be false, those unpersuaded by consistent histories had reached no agreement on precisely what constitute the problems — still less the virtues — of the consistent histories program.

This article aims, no doubt optimistically, to explain both the problems and the virtues, to persuade consistent historians that their formalism is scientifically problematic, and to persuade sceptics that it is nonetheless scientifically interesting. I will try to show that the consistent histories formulation, sensibly interpreted, significantly changes our understanding of the scientific status of quantum theory, not only because it offers a new formulation but also because it sheds new light on earlier interpretations. In particular, I will compare and contrast the consistent histories approach, the Copenhagen interpretation à la Landau-Lifshitz, and Bohmian mechanics. In so doing I will argue that some important technical claims made in the consistent histories literature turn out to be simply false. When sensibly interpreted, the formalism's chief virtue turns out to be not, as advertised, that it solves the problems of quantum theory, but rather that it highlights particular scientific problems. Nonetheless, I will conclude, it can be used to give an interpretation of quantum theory which in important ways is better crafted, or at least more honest about its deficiencies, than any of the standard interpretations.

This discussion of consistent histories is drawn from recent joint work with Fay Dowker.[1,2] Our conclusions are set out at length in Ref. [1]. Rather than repeating the details of the arguments here, I will try to state the main results succinctly, to add some explanatory comments, and to respond to some points raised during the Bielefeld meeting.

Our thesis is the following. We agree with other critics of consistent histories that the present interpretations of the formalism have some extremely unattractive features: indeed, we show that in the cases of Omnès and Gell Mann and Hartle they have more serious problems. However, we distinguish between criticism of the interpretations offered in the literature and criticisms of the consistent histories approach *per se*. To take one example, Griffiths' proposal to interpret the formalism as defining a non-classical logic is, of course, open to the usual criticisms of quantum logic. This seemed to cause confusion at Bielefeld: some took Griffiths' logic to be an essential part of the consistent histories program. Yet nothing in the consistent histories formalism requires it. On the contrary, the formalism defines a sensible interpretation of quantum theory, using ordinary logic and

language, which we call the Unknown Set interpretation. It is instructive to examine this interpretation, to see why it cannot be improved upon without going beyond the consistent histories formalism, and to understand its weakness as a scientific theory — for one is then forced to appreciate both that the consistent histories formalism has virtues which other approaches to quantum theory lack, and that it has defects which other formulations remedy.

Let me now try to explain the reasons for our conclusions, assuming familiarity with the basic notions of consistent histories. For definiteness, consider the non-relativistic formulation in which sets of consistent histories are defined by sequences of projective decompositions  $\{\sigma_1, \dots, \sigma_n\}$  at times  $t_1, \dots, t_n$ , each  $\sigma_i$  comprising projections  $P_i^1, \dots, P_i^{n_i}$ , together with the Gell Mann–Hartle consistency conditions

$$\text{Tr}(P_n^{a_n} \dots P_1^{a_1} \rho_i P_1^{b_1} \dots P_n^{b_n}) = \delta_{a_1 b_1} \dots \delta_{a_n b_n} p(a_1, \dots, a_n), \quad (1)$$

this last expression defining the probability of the history. Here  $\rho_i$  is the density matrix defining initial conditions for the system, which I take to be the universe. There are other interesting formulations and consistency conditions in the literature. Moreover, the formalism admits a time-symmetric generalisation of quantum mechanics in which a second density matrix  $\rho_f$  defines final conditions. However, so far as we can tell, the basic scientific problems of the formalism are unsolved by any of these variations.

When the possible sequences of projective decompositions are suitably parametrised, the Gell Mann–Hartle consistency conditions reduce to simple algebraic equations in the parameters. In other words, once the boundary conditions are fixed, the classification of consistent sets is a purely algebraic problem. It is hard to solve the relevant equations in any but the simplest of examples, or to prove general results about their solutions. Assume for the moment, though, that the equations have no very special properties. One would then expect that almost all the solutions can be parametrised by a number of parameters equal to the number of unknowns minus the number of consistency equations. If the number of parameters is much larger than the number of equations, one would also expect

that, given an approximate solution, one can generically find an exact solution very close by.

I mention these mathematical trivialities because, if they apply to consistent historical descriptions of real world physical events or experiments, they have interesting consequences for our understanding of the theory.<sup>1</sup> Suppose, for example, we set up a series of  $N$  independent experiments in which (to use the standard Copenhagen language) distinct macroscopic devices measure observables of a microscopic quantum system and display the results by pointers, the experiments being complete and their results displayed at times  $t_1, \dots, t_N$  separated by macroscopic intervals. Consistent historians can, of course, reproduce the standard probabilistic predictions for the results of these experiments, and do so roughly thus. First, we identify the initial density matrix. Next, we fix orthogonal projection operators at times  $t_1, \dots, t_N$ , corresponding to the possible positions of the relevant pointers and their complement. Then we argue that the decoherence effect of the environment (photons interacting with the pointers, and so on) will ensure that the set defined by these projections satisfies the consistency conditions to an extremely good approximation — the off-diagonal terms, let us say, are no larger than  $10^{-40}$ . Finally, we take this degree of inconsistency as completely negligible, and simply use the standard decoherence functional expressions for the probabilities of the various results. The justification for this procedure, given by Gell Mann and Hartle, is that we need not require a fundamental theory to give precisely defined probabilities, or to give probabilities which precisely obey the standard sum rules, since the purpose of theory is to calculate testable quantities and errors of  $10^{-40}$  in our probability calculations are inconsequential in any conceivable experimental test.

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<sup>1</sup> Unfortunately, though it would be surprising if this discussion did not apply to real world physics, there seems to be no way to test the question directly. It would, though, be interesting to test whether the consistency equations do indeed have the expected algebraic properties in moderately sized Hilbert spaces and our conclusions hold in toy models.

If our expectations about the consistency equations are justified, we can improve on this discussion. The number of parameters needed to parametrise possible projection operators is hugely — perhaps infinitely — greater than the number of consistency equations here, and this has two important consequences.

The first is that the approximately consistent set used in these calculations could, in principle, be replaced by a very similar exactly consistent set which would produce essentially the same probabilities — indeed, we expect a parametrised family of exactly consistent sets passing close to the set we initially used. We do *not* expect the sets in this family generally to involve projections we would naturally consider: their projections will generally be onto complicated subspaces of the Hilbert space describing the apparatus and its environment. Nor do we expect any single set in the family to be picked out in any natural way. Nonetheless, the consistent histories formalism tells us that the family contains valid sets of histories with well-defined probabilities. Thus there is no need in principle ever to introduce approximately consistent sets: we can assume, without any serious fear of experimental contradiction, that exactly consistent sets are the only ones of fundamental physical relevance. Of course, this makes no practical difference, since we do not know precisely *which* exactly consistent set is relevant to any given experimental or cosmological calculation, and in practice — unless and until some rule is found which identifies the relevant consistent set for us — we would generally use the usual approximately consistent set and accept that we thereby introduce small errors. Nonetheless, on this view, the formalism defines a mathematically precise theory, and this — if elegance and lack of ad hocery are thought to be of any intrinsic merit — must surely be counted a gain.

The second and perhaps more significant consequence is that the consistent histories formalism shows that the standard Copenhagen description is chosen from a far larger class of possibilities than we previously appreciated. For we expect the parametrised family of consistent sets to be characterised by a very large number of parameters, and to be dominated by exactly consistent sets very far away from the approximately consistent set we chose initially. Now it has certainly always been understood that there are slight

ambiguities in any Copenhagen description of a series of experiments, since there is a certain freedom in the choice of the Heisenberg cuts between system and observer. What is new here is the discovery that there is a continuous family of equally valid physical descriptions of the experiments and that, while this family includes standard Copenhagen descriptions, almost all of its members involve variables quite different from, and not even approximately deterministically related to, the classical degrees of freedom used in the standard discussions. In the Copenhagen approach, any assignment of probabilities to physical events not describable by classical degrees of freedom is forbidden. If we accept the consistent histories formalism as a correct generalisation of the Copenhagen interpretation, we have to accept that such an assignment is theoretically sensible, and we then have to understand why the Copenhagen interpretation is nonetheless all that we need for practical purposes.

This, in fact, is the key question. The formalism offers a myriad of possible variables for describing physics. Can it, suitably interpreted, explain why the world reliably continues to appear to us always to be described by the particular measure zero subset corresponding to familiar quasi-classical variables? Dowker and I argue that it cannot.

In fact, we make the following stronger claim. The scientific content of the consistent histories formalism is given by the so-called Unknown Set Interpretation, which postulates that the fundamental probabilistic theory of nature is defined by a choice of initial density matrix, hamiltonian and canonical variables — all of which we might hope to specify precisely by some elegant theory — together with some unknown and theoretically un-specifiable consistent set of histories. The histories from that set define the sample space of possible events, and the decoherence functional then defines the probability measure on that sample space in the usual way. Thus, one history from the Unknown Set is chosen randomly to be realised, and it is this history which describes all of physics.<sup>2</sup> No other

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<sup>2</sup> It is often suggested that a fundamental theory which assigns probabilities to a single event in this way is problematic, or even meaningless. This is usually intended to be a criticism only of a particular type of theory, but seems in fact to imply a rejection of all probabilistic physical theories. For, practically speaking, a theory phrased in this way is no more or less testable than any other probabilistic theory, since we can only perform finitely many experiments.

history, from this or any other set, is realised, and no set other than the single Unknown Set is of any relevance for calculating the probabilities of physical events.

Let me emphasise at once that we are not suggesting that this interpretation is ultimately satisfactory. We put it forward to strip away what we see as inessential and sometimes confusing proposals in the literature. Our claim is that, insofar as the consistent histories literature supplies sensible interpretations of the formalism, those interpretations are almost precisely scientifically equivalent to the Unknown Set Interpretation — in other words, they make almost precisely the same predictions, retrodictions and inferences. Where the literature claims to go beyond these predictions, retrodictions and inferences, it is either erroneous or else relies on significant assumptions extraneous to the consistent histories formalism.

Before making the case that the Unknown Set interpretation really does encapsulate the scientific content of the formalism, let me discuss its scientific implications. An interesting question, raised at Bielefeld by Michael Dickson, is whether so apparently weak an interpretation really deserves the title of a scientific theory. To give a fully satisfactory answer would require a general set of criteria for scientific theories. I do not have such a set of criteria. However, it seems to me that, once the initial density matrix, hamiltonian, and canonical variables are specified, the interpretation ought comfortably to pass any reasonable test. It is well-defined, and moreover comes from a quite elegant and natural mathematical formalism; it is certainly falsifiable; one even, at present, has to admit (as a matter of logic rather than of plausibility) the possibility that it is the best purely mathematical theory of nature which can be constructed. The interpretation makes one definite prediction, which is that all the events we have observed to date, or will observe in the future, can be described by a history of non-zero probability from some consistent set. If this fails to hold, as in principle it could, then the interpretation — and, of course, the entire consistent histories formalism — must be rejected. The interpretation makes further predictions. These predictions are generally probabilistic, in the same way as the Copenhagen interpretation is. What is new and peculiar to the consistent histories formalism is that they are also conditional on an unknown physical quantity: the relevant consistent



set. Given a particular observed history, we can predict that if a certain event is described by one branch of a consistent extension of that history, and if that particular consistent extension turns out to be part of the Unknown Set, then the relevant event will occur with the conditional probability defined in the usual way by the decoherence functional. If — when, say, we attempt to predict the outcome of a series of experiments — we find that we do observe definite results (i.e., in this interpretation, that the corresponding projections do belong to the Unknown Set) but that the calculated conditional probabilities predict outcomes significantly different from those observed then, again, we must reject either the theory being interpreted — i.e. the specification of boundary conditions, hamiltonian, and canonical variables — or the interpretation itself. Likewise, even before we perform experiments, we are likely to reject some aspect of the theory if the observed history to date is highly improbable. Like most probabilistic tests, these last two of course require some intuitive or theoretical method of coarse-graining events. That is, we reject the theory not because a small probability event occurs (all possible alternatives may have small probability) but because we believe we can identify a natural division of the alternatives into two classes and we find that sum of the probabilities of the events in the class to which the occurring event belongs is small.

The problem with the Unknown Set interpretation — and, of course, the reason for doubt as to whether it constitutes a theory — is that it gives no algorithm for making probabilistic predictions which depend only on the observed data. Every prediction takes the following form: “*if* the Unknown Set contains the following projective decomposition at the following future time, *then* the probability for the future event described by one of the projections is  $p$ ”. The interpretation does not predict that any future events will occur, or that those which do occur will be describable in terms of familiar variables. In particular, it does not predict that those variables which Gell-Mann and Hartle call quasi-classical — variables which describe macroscopic aggregates and which generally follow deterministic equations of motion to a very good approximation — will continue to be relevant. Quite the contrary: according to this interpretation, the apparent persistence of quasiclassicality is a great and inexplicable mystery. Thus, granted that the interpretation defines a scientific

theory, it is a theory with a glaring weakness. For most physicists, surely, believe that they *will* continue to experience a quasiclassical world for the foreseeable future: few are startled each morning by the dawning of yet another quasiclassical day. The persistence of quasiclassical experience, in other words, is part of our theory of nature. Some well-known presentations of quantum theory assume it explicitly.[3] Many do not, apparently because it has not been understood that there are well-defined interpretations of quantum theory in which quasiclassicality would not be perceived to persist, and that we need some scientific reason for rejecting such interpretations. A virtue of the consistent histories formalism, in the Unknown Set interpretation, is that it makes these points absolutely clear.

Let us now turn to the interpretations in the consistent histories literature. It is only possible to outline the arguments here, but perhaps a brief precis will be of use. I hope the reader, and those criticised, will forgive the necessarily crude summaries.

Griffiths[4] suggests that the consistent histories formalism should be interpreted as defining a new logic adapted to propositions describing the physical world. Griffiths' logic has the property that any two propositions referring to projections belonging to different consistent sets can be true without implying that their conjunction is true. We can, for instance, predict that the detectors at CERN will function tomorrow in the ordinary way, producing quasiclassical records of the events they detect, and also predict that the detectors, and their recording devices, and much else besides, will *not* behave quasiclassically tomorrow. We cannot, however, use Griffiths' logical rules to deduce the prediction that the detectors both will and will not behave quasiclassically. We hence avoid contradiction though — as is usual with quantum logic — at the price of a theory which we simply do not understand how to interpret. Griffiths' interpretation, however, skirts the key point. We can never experience the truth or falsity of propositions from more than one consistent set. If the formalism is fundamentally correct then all our scientific endeavours will be described by one consistent set and the scientifically relevant problem is the identification of that set. We can, of course, do calculations in other sets; we can too, if we wish, manipulate propositions involving other sets according to Griffiths' logical rules — but neither of

these activities are of any use in predicting the future we will actually experience. Griffiths' interpretation is scientifically equivalent to the Unknown Set interpretation.

Omnès also interprets the formalism as defining rules for the logical analysis of propositions about the physical world.[5,6,7] Omnès' logics are conventional: propositions belonging to incompatible consistent sets simply cannot be discussed together. The significant new proposal in Omnès' interpretation is the notion of a "true proposition" — a proposition which is not given to us in the form of observed data, but is deducible from those data by a new rule appended to the consistent histories formalism. Unfortunately,[1] as Omnès accepts, the rule he originally proposed fails to allow the intended deductions: indeed, it generally seems to allow almost no deductions.<sup>3</sup>

Perhaps it is a slight overstatement to say that we are left with an interpretation scientifically equivalent to the Unknown Set interpretation: this depends on exactly how narrowly one defines science when its subject matter is the past. Any principle which allows even a few inferences about the past, untestable though they may be, would probably generally be regarded as scientifically useful if those inferences form part of an elegant and compelling theoretical explanation of present data. There seems to be no evidence that any inferences implied by Omnès' original criterion do so, but the possibility cannot be completely excluded. However, so far as the criteria for truth in the existing literature are concerned and insofar as they apply to predictions, we must indeed conclude that they indeed do not affect the scientific status of the formalism.

Gell Mann and Hartle's conclusions,[8,9,10] however, certainly go beyond those implied by the Unknown Set interpretation. In fact, Dowker and I argue[1] that those conclusions are not entirely coherent in their use of the formalism. Nonetheless, the central claim of Gell Mann and Hartle's interpretation is tenable. This is the suggestion that quasiclassicality appears to us to persist not because quasiclassical variables play any special role in the

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<sup>3</sup> Could another definition of "truth" do the job? Omnès has new proposals. Dowker and I too have investigated possible alternative rules. Our tentative conclusion is that interesting rules do exist which allow at least some non-trivial inferences about the past, but we can identify no rule which allows any useful predictions of the future. We hope to give a detailed discussion elsewhere.

theory, but because we ourselves have evolved organs of perception which are sensitive to those variables and a mental apparatus which represents the world in quasiclassical terms. But is this a valid argument without further assumption, or is it, like other recent ideas relating consciousness to quantum theory, a speculation?

At first sight it appears not only a valid argument but close to a truism. Almost all scientists would agree that our perceptions and our mental algorithms have evolved to become highly sophisticated at gathering and utilising quasiclassical data. This agreement, though, is predicated on the assumption that one may assume a quasiclassical description of the world. Quasiclassical variables arise naturally in higher order theories of nature such as classical mechanics, chemistry, and terrestrial biology, and evolutionary biologists take their use for granted. Likewise, our theories of brain function are classical theories and our understanding of consciousness, such as it is, is entirely based on classical models. We cannot use biological science to justify any general conclusions about evolution, perception, or consciousness from within a novel interpretation of quantum theory such as the consistent histories interpretation. For if we take seriously a theory — such as the consistent histories formalism — which describes us as being in superpositions of quasiclassical states, or in states defined in terms of entirely non-quasiclassical variables, we can make no statement about our perceptions in those states without new hypotheses. Such hypotheses would necessarily be speculative: they certainly do not follow from our conventional, quasiclassical understanding of the relation of perception to brain function; nor do they follow from any empirical data or theoretical insight presently available to us. What one would need, in fact, is a theory of consciousness written directly in the language of the formalism. It is hard to imagine, and Gell-Mann and Hartle do not try to explain, how one would presently go about trying to formulate such a theory.

We conclude, then, modulo a minor caveat about Omnès' treatment of the past, that the Unknown Set interpretation is indeed scientifically equivalent to the interpretations discussed in the literature, when they are stripped of extraneous hypotheses. Although there is currently no single canonical formulation of quantum theory, it can reasonably be argued that the consistent histories formulation is the minimal formulation of the quantum

theory of a closed system which produces a well-defined scientific theory. It tells us that, even when we ignore general relativity, our theory of the macroscopic world — and in particular our expectation of its persisting quasiclassicality — involves assumptions that go beyond both quantum theory and any theory of the cosmological boundary conditions. Some thoughtful critics and advocates of orthodox quantum theory have long appreciated this. Perhaps it has remained controversial only through the wider confusion over interpretations of quantum theory. The consistent histories formalism now so clearly defines a natural interpretation of quantum theory, and spells the conclusion out so precisely, that it is hard to see how any serious controversy can persist.

As we have seen, the formalism also shows that there are equally valid, perfectly well-defined, alternatives in which quasiclassicality persists only for an interval, or never arises. This is an important new development in our understanding of quantum theory.<sup>4</sup>

Bohmians and collapse model theorists may happily accept that the formalism supplies new arguments against orthodox quantum theory, but will perhaps feel our analysis confirms their belief that the formalism has no positive scientific use. Is the formalism not, after all, scientifically sterile? Were the problems it illustrates not solved long ago by Bohmian mechanics? Are dynamical collapse models, with their intriguing alternative explanation of quasiclassicality, not a far more vital subject of research? I sympathise with the spirit of these questions, but let me end by explaining why I cannot dismiss the formalism so conclusively.

First, while defining the problems of quantum theory very clearly, it also suggests an interesting possible form of a solution. All one needs is a rule (perhaps probabilistic) which

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<sup>4</sup> Its novelty might be disputed. It is true that something similar occurs in interpretations of quantum mechanics in which the events at different times are entirely uncorrelated. For such interpretations arbitrary basis selection rules can be used at each point in time, and in particular one can use rules in which the system lies in an eigenstate of quasiclassical operators for a while and an eigenstate of non-quasiclassical operators thereafter. But few take such ahistorical interpretations seriously. One can probably find historical interpretations, other than the consistent histories formalism, in which quasiclassicality does not persist — for example, it ought to be possible to produce generalised Bohmian theories with this property — but I can think of no discussion of such interpretations in the literature.

takes as input the dynamics and boundary conditions of a theory and produces as output a consistent set (the Unknown Set) which turns out to be quasiclassical. This, of course, begs the question of whether such a rule can be found. Here the superiority of Bohmian mechanics and of GRW-type collapse models is presently clear, since the analogous selection principle is already known in both cases and both theories explain quasiclassicality. It remains to be seen whether they give the right explanation and whether they are capable of giving any explanation in the context of relativistic quantum field theory: the same, of course, is true of the consistent histories formalism.

Second, the consistent histories formalism seems to be a strong competitor theory where cosmological applications are concerned. For example, one can easily imagine theories of structure formation involving a series of past events which can be described within a consistent set but which, even if a good Bohmian cosmological theory were to exist, could not naturally be described in terms of Bohmian trajectories.

Third, the consistent histories formalism surely ought to be explored further precisely because it is a good formulation of quantum theory. If the eventual goal is to go beyond quantum theory, it is probably as well to understand all interesting formulations and interpretations of the theory. Different formulations, after all, may inspire different post-quantum theories. We need, in particular, to understand the possible definitions of “truth” and their properties; a general treatment of quantum field theory in the consistent histories formulation; and a clearer understanding of how the formalism applies to cosmology.

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